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DISCUSSION.

REMARKS ON PROF. LEUBA'S CRITICISMS.

The INTERNATIONAL JOURNAL OF ETHICS is not the place to carry on a too elaborate discussion regarding a technical question in abnormal psychology, but since I have been offered the opportunity for some remarks upon Prof. Leuba's criticism, in the October number, of my Report on the Piper case, it may not be out of place to comment upon the matter briefly. I must remark, however, that his frank recognition of the scientific character of the work, the admission that some of the incidents are at least suggestive of something unusual, and the evident care with which he has read the detailed record, something which it is hard to induce psychologists generally to do, make it an ungrateful task to enter into any defence of my interpretation of the phenomena in that Report. In fact, I shall not here say a word in defence of the spiritistic theory, as I do not think this defence is nearly so important as the work of experimenting and putting into scientific shape and record the facts that purport to have some sort of supernormal explanation. It was due to the apparent character of the facts to test the theory which they suggest, while the most important task was to have scientific method applied to them in both the record and the analysis, whether the conclusion presented is ever verified or not. All that I shall endeavor to do is to correct various errors of statement and representation which might prevent the reader of Prof. Leuba's discussion from going to the Report to judge the facts for himself. There are some general remarks to make before going into details.

1. The reader will observe in a careful reading of Prof. Leuba's criticism that his general treatment of the incidents labors under a serious misunderstanding of the grounds on which many of the incidents were spoken of as true. In spite of his recognition that I was not attempting to demonstrate spiritism, Prof. Leuba has treated many of the incidents as if I were assuming them to be evidence because they were true. I was very careful to say explicitly (pp. 116-117, 293) that I was *testing*, not *proving*, the spiritistic hypothesis. I had before

me the alternatives of fraud, secondary personality, and telepathy, with perhaps the conjunction of the last two. Refusing to discuss the first, it was my duty, after showing that subliminal guessing had been excluded along with suggestion from myself, to examine every fact, important or trivial, with reference to its explicability by the hypothesis which I had proposed to test. This policy does not imply that I was treating every true incident as evidence. I explicitly said that I did not so treat them (p. 293). If I had been demonstrating the spiritistic hypothesis I should have been obliged to isolate the evidential from the true non-evidential incidents, and to have allowed the latter to be explained by other suppositions. But I was dealing with both an evidential and an explanatory problem. The evidential aspect may require discrimination between the various types of data offered, but the explanatory aspect will not permit this. The test of an hypothesis is what it will consistently explain throughout the total mass of material. If it will not explain certain facts it is defective and some other hypothesis has to be substituted. I should have done a very foolish thing, when avowedly testing a theory, if I had only picked out the evidential incidents and ignored those which were equally explicable by it though not qualified to prove it. Prof. Leuba has evaded the conception of the problem which I had explicitly formulated and treated the details as if I was regarding many of them as evidence because they were true.

2. Probably the most important reason for Prof. Leuba's failure to see the point of view from which I was discussing the problem is the circumstance that he has not remarked the point of view from which the spiritistic hypothesis was conceived. I had explicitly stated (pp. 16-17, 246, 250) that I had formed my conclusion upon a wider record of facts than my Report, as I was in duty bound to do, and was simply applying the hypothesis so obtained to the incidents of my record to test it in detail. Prof. Leuba does not reckon with this fact in any respect, and tacitly or openly implies that I was assuming incidents to be evidential which in fact I had no more treated as such than he would do. In testing an hypothesis, all facts have to be treated alike; in the true, both the evidential and the non-evidential, and in the false, their consistency with the main hypothesis through rational adjuncts.

3. The whole weakness of Prof. Leuba's position is apparent in his treatment of what is false in the record. He calls attention to the false incidents without for one moment asking the question how they can, in the form in which they occur, be compatible with any of his hypotheses. Nor does he make it clear to the reader, as he should have done, that I had fully considered the false incidents. In fact, Prof. Leuba says: "The writer of the Report goes over the incidents which have, or might have, evidential value; the rest, *i. e.*, the false and irrelevant, is passed over unmentioned." I was especially careful throughout the Report in my Notes and Summaries to call explicit attention to the false incidents, so that I should not be charged with this very neglect, and I gave a most careful discussion of them in their relation to the spiritistic hypothesis (pp. 214-238). The reader will note that, in stating the "main advantages" of the spiritistic theory, Prof. Leuba omits to mention this consideration of "Mistakes and Confusions," though he specifies the arguments from the "Unity of Consciousness" and from the "Dramatic Play of Personality."

4. Almost every objection which Prof. Leuba presents against the Report was anticipated and answered by the experiments recorded in Appendix IV of the Report (pp. 537-623), and there is not the slightest evidence in Prof. Leuba's discussion that he has even looked at this set of experiments. My position and the evidential question will not be appreciated until these experiments are studied with the same psychological care that is due to the experiments with Mrs. Piper, and which Prof. Leuba has read with more care than the usual psychologist.

5. The close of Prof. Leuba's discussion makes it very clear that he thinks, if the spiritistic theory be adopted, the facts attest such a meagre existence that most of us would wish to be delivered from it. In this view he shows very distinctly what he missed by not examining Appendix IV with reference to this very point; for I there record experiments undertaken for the very purpose of showing that rational living persons would choose incidents to prove their identity that indicated nothing of their normal life at the time, and the result showed that, if Prof. Leuba's mode of interpretation and remark have any justification whatever, he would have to conclude that the "communicators" in those experiments, professors and students in Columbia University, were insane, and that the remarks and

defective memories of some of the receivers even are laughable. Prof. Leuba's observations expose him to the accusation of not having studied the nature of the evidence which will prove personal identity and exclude fraud, chance, and suggestion.

6. Prof. Leuba notes what many critics neglect to observe in my discussion of the spiritistic theory, namely, that a most essential feature of it is the view that abnormal mental conditions prevail on the "other side" as necessary for communication, but he regards the view as a "damning supposition," though it seems essential. But Prof. Leuba does not indicate a single scientific reason for thus rejecting the supposition. He gives it a merely *ad captandum* rejection, with the assurance that the scientific plebs will accept the rejection without question. But what evidence has he that such a supposition is false? How would he prove a negative here? I grant that the supposition cannot be made without reasons, but it is equally certain that no man has any right without equally explicit reasons to deny or ridicule it. I was very careful to point out both *a priori* and empirical reasons for my supposition (pp. 284-285, 643-649), and this fact should have been recognized in any statement of my position. I showed that the conditions must be complex and not simple, and that we have no reason to suppose that the conditions for communication have the stability of our mechanical methods of communication through space. To have all messages clear and free from mistake there would have to be a constant and perfect adjustment between three sets of conditions: (1) The physical and mental condition of the medium; (2) the mental condition of the "communicator;" (3) the relation between two supposed modes of existence which must render communication more difficult than between living persons. Prof. Leuba forgets to note that, without motor vocalization, very little intelligent communication can be mediated between the living, and that little only by means of common experiences and symbols previously agreed upon. All communications from a transcendental world must involve some effect in our physical world and the most probable source of such effects must be the human organism, and there is no guarantee that this should be a very perfect instrument for any consciousness but the incarnate. It is more than possible that such a medium would offer almost insurmountable obstacles to any communications whatever, to say nothing of their being clear and without mistake. Prof. Leuba

shows an inclination to demand a greater freedom from error in the message than any scientific man can make. The proper duty, however, of the scientific man is to explain the true facts that are obtained and not to make up his mind against the case on the ground of what is not obtained. If the positive evidence justifies a belief in the supernormal, the mistakes and omissions are not objections, but an additional problem.

7. In rejecting the argument from "selectiveness" against telepathy Prof. Leuba wholly misunderstands my statements about it and the point of view which I took in it and which every psychologist must take in it when estimating telepathy. I grant all the "selectiveness" you please for secondary personality when you are dealing with the subject's own experiences. But it is another thing when you are dealing with telepathic subjects. Secondary personality means that your selected content is mnemonic in its nature or constructive along the lines of the subject's normal experience. Telepathy, assuming it possible, involves access to other minds, and if it is selective here it is not confined to the present active states of consciousness, as it would be on mechanical or dynamic principles. The selectiveness about which I was talking did not mean that the selective process was carried on after the telepathic acquisition of the fact; for this would imply that much more had been acquired. But it meant that the telepathic process, if admissible as an explanation of the record, had to be supposed to imitate exactly what a surviving consciousness would necessarily be expected to do, and then face mistakes incompatible with the omniscient powers assumed to explain the facts actually selected. Prof. Leuba does not meet this issue by referring to the selectiveness of secondary personality, which I had explicitly granted and had even expressed the willingness to regard the trance personalities as nothing else.

8. In the matter of estimating evidence Prof. Leuba has totally evaded the fundamental value of confused incidents and the possibility of establishing personal identity on them as well, or even better than on other and clear facts. There are no better means of discrediting chance, suggestion, and fraud than by certain types of confused and erroneous messages. Prof. Leuba's criticism along this line shows no empirical evidence upon which to base his judgment. I had anticipated his whole plan of criticism and met it by special experiments to show that

confused incidents and the barest hints, far less specific than in the Piper phenomena, were sufficient to establish identity and to exclude chance and suggestion. This was in Appendix IV, not noticed by my critic, where occasional instances of fragmentary messages between living persons succeeded well and the most specific and clear messages failed (pp. 586-590). The same phenomenon is frequently observable in this record between pp. 608 and 623 inclusive. I called special attention to this fact on p. 268 of my report and italicized one very interesting instance bearing upon the point at issue, namely, "Question 7, p. 619," where the confusion was worse than in many of the Piper incidents and yet was the first source of identification.

9. There are a number of gratuitous references to "affective influences" and "unconscious influence" which it would be hard to prove, but which are more useful for imputing weakness than for making out a scientific argument on the other side. It is strange that scientific men always despise emotions when they are in favor of religion or a future life, but do not reckon with them in skepticism. I have never seen a monopoly of cool judgment on the side of doubt, but scientific men like to play Stoics in religious matters when this is not their attitude in any other relation of life. It seems to be good *ad populum* argument to impeach spiritists for "affective influences" without empirical evidence that this applies in the concrete case, assuming all the while that skepticism is an unprejudiced judge. Apropos of this assumed exemption of the skeptic from "affective influences" my critic says: "Let us rather listen to the conclusion of those who have only read and not witnessed." What kind of science is this? Here is advice to scientific men not to investigate and experiment for themselves, but to condemn *a priori* those who do! This is all the more remarkable from the fact that it comes from a man who is himself a psychologist and who is not supposed to form any convictions without personal investigation and experiment.

10. I should notice a number of misconceptions and errors of statement which the reader of the discussion would not remark without a careful study of my Report.

In regard to the incident about "tokens" Prof. Leuba's interpretation that it refers to "token of friendship" is not borne out by the text. It is invention pure and simple, arising out of an association in his own memory about the significance of

the word, while the text alludes to the "communicator's having tokens which he *recollects* and in the next sentence refers to a photo in his "collection." If Prof. Leuba does not want me to complete the incidents to which the "communicator" apparently refers, he must not add to the actual record of facts ideas that are neither expressed nor implied in it. This is evading the force of the facts by invention. Then, again, I did not say or imply that it was a "wonderful incident." I only said that it was "interesting."

Prof. Leuba's remark that the second passage quoted by him "seems clearly within the possibilities of invention" is subject to the following comments: (a) It is not found on the pages mentioned; (b) I nowhere called it "remarkable," as I am said to have done; (c) It was not deemed or represented by me as such; (d) My first notes rejected the passage as false or nonsense. More careful inquiries proved many of the incidents true and I merely stated this fact without regarding them as either remarkable or evidential. The passage referred to was deemed of so little value that I did not even refer to it in the summary of facts. (e) The question why the "communicator" did not specify more clearly the special use of his paper cutter is answered by what I said on pp. 214-238, 249, 284-285.

If the reader will examine the record carefully he will observe that Prof. Leuba's remarks about the McClellan incidents are largely *a priori*. No reasons are given for his rejection of their pertinence, while contextual considerations are wholly ignored by him. Appendix IV shows identification of personal identity on far less pertinent evidence and without the giving of any proper names. I am quite content to let any student who is familiar experimentally with such incidents decide between Prof. Leuba and myself on the supposition that the McClellan incidents are due either to chance guessing or suggestion.

My own position is that the only hypothesis which can give any difficulty to the student is that of conscious fraud, and I only wish it distinctly remembered that I refused in the Report, and still refuse, to discuss that hypothesis until some one endeavors to apply it in detail, with the adjuncts necessary to make it probable and intelligible. I had carefully considered suggestion and guessing and would not waste time in applying them in detail (pp. 16-17, 124, 247-248). I consider that every intelligent man who experiments on this subject of guessing and sug-

gestion will rather take refuge in fraud as the best means of giving trouble in this problem.

On the whole, I cannot complain of any desire by Prof. Leuba to do the Report an injustice. I am rather pleased with the general spirit that is shown toward such work, and I am not anxious to make proselytes. Every man must make up his own mind on the problem and scientific psychologists cannot adopt the hypothesis which I have defended until much more has been accomplished in this perplexing field, and until they have the opportunity to come into direct personal contact with experimentation in it. But I will say that there will be little opportunity granted them as long as they publicly ridicule it. There are no doubt fakirs and silly performances have given the conception by which the average intelligent man and scientist has to judge of the subject, and the contempt with which this has to be met only make respectable people conceal genuine phenomena from the knowledge of scientific men. The only way to find facts which really require investigation is to exhibit the kind of tact and respect which the student of abnormal psychology has always to show in the treatment of pathological phenomena. When this is done, our psychologists will find it convenient to avoid public ridicule of the subject and they may actually discover some interesting facts right at home.

JAMES H. HYSLOP.

NEW YORK.

REJOINDER TO PROF. HYSLOP.

I regret very much the erroneous statement of which Prof. Hyslop complains in Section 3 of his answer. I do not know how to account for it and I cannot now do better than offer an apology.

The recognition of the mistake just referred to does not, however, modify my opinion that the first part of the volume gives a wrong impression, *i. e.*, an impression much more favorable to the spiritistic hypothesis than the one derived from the complete records filling the second part of the book. I had simply stated my own observation and that of other persons. I have since then verified its correctness. But, as I had said no blame attaches to Prof. Hyslop on that score.